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## A Critique of Psychophysics Methods.

I READ with care the comment by Dr. Boas upon my article in the *American Journal of Psychology*, and carry away from it the impression that there is less difference of opinion between us than Dr. Boas supposes. The question is not one of fact, but of interpretation. We all admit that there is a psychophysical fact for which the word 'threshold' is a good name; but the important question is, How shall we theoretically understand the conception, and what place shall we allow it in the development of an experimental psychology? Fechner makes it rank as by all means the most important factor in psychophysics, and is willing to sacrifice Weber's law before yielding the supreme and fundamental fact of the threshold. He is led to this view by the method of the 'just observable difference,' and by the neglect of the other two methods. This entire structure I regard as reared upon an illogical basis, and a psychophysics based upon the mathematical methods as very different and much sounder than the other. The threshold as a practical, empirical fact, I not only fully admit, but even suggest methods of further developing its utility; but its theoretical importance with reference to the establishment of a psychophysical law I regard as almost *nil*, its true importance lying in another direction. This, I trust, defines my position clearly. A single illustration may not be out of place. Dr. Boas says that a balance has a threshold, and I accept the comparison. This threshold is something to be eliminated, and that balance is the finest that has the least of this characteristic. The theoretical balance upon which mechanics works out its principles has no threshold. But apart from this, I think the physicist will agree with me that it leads to more useful and scientific conceptions to regard every particle that is placed upon the pan of the balance as producing an effect alike in kind, and differing only in degree from that produced by a mass sufficient to turn the balance. There is no point where a new factor enters, and the turning of the balance is a merely empirical fact. Returning to the psychophysical methods, I should state the case thus: it is generally admitted that the basis of the method of the "right and wrong cases," as of the "average error," ultimately rests upon the fact that the probabilities of my making errors of various degrees follow the path traced by the probability curve. This is the fundamental fact of the entire science of psychophysics. Now, this curve is a *continuous* one, and has no break in it, no point characterized by any special peculiarity, no threshold in any true sense.

A word as to my misrepresenting the views of my opponents. The important point is, not what the upholders really do say, but what logically follows from the position they take. If they do not say what I attribute to them, it is because they are inconsistent; and I have guarded myself against this misunderstanding by at times stating, and elsewhere unmistakably implying, that I was dealing with the logical consequences of the threshold theory, and not with that particular portion of it that its adherents happened to employ.

The second point in Dr. Boas's criticism is a real difference of opinion between us. He thinks "doubtful" answers should be admitted in experimentation: I most emphatically object to them. In my paper I regarded the objections to allowing such answers as so necessarily following from the theory of the "right and wrong cases" method, that a full statement of the reasons was superfluous. Any one of half a dozen reasons is enough to show the impropriety of the "doubtful" answers. For instance: it is admitted that the methods should be as comparable, one with the other, as possible. Now, the method of the "average error" depending upon the same principle as that of the "right and wrong cases," allows no doubtful answers. Again: there is no reason for singling out "doubtful" answers as any thing peculiar. Why not make a special rubric of unusually confident answers? And if we do, as Dr. Boas suggests, make a threshold where doubtful answers no longer occur, that threshold will vary so much in different individuals, etc., that it will invalidate a large share of the results. And what shall I say when some one else proposes a threshold for another degree of confidence, say, the point where one is sufficiently sure of the correctness of one's answer to risk money upon it, and so on, *ad infinitum*? If you mean that this subjective feeling is worth taking account of, I fully concord, and will wel-

come the skilful observation of this feeling as an important contribution to psychophysics.

JOSEPH JASTROW.

Baltimore, March 12.

On the Sense of Taste.<sup>1</sup>

AT the Philadelphia meeting of the American Association we presented a paper upon the 'Delicacy of the Special Senses,'—a topic upon which we have since continued our investigations from time to time.<sup>2</sup>

The method pursued in the following experiments was as follows:—

Solutions of known strength were made of the substances to be tasted; then, by successive dilutions, several series of solutions were made from these, each one in the series being of one-half the strength of the preceding one. The bottles containing these solutions, and several bottles of water, were placed without regard to order, and the person to be experimented upon was requested to separate them into their proper groups by tasting them. In each series the last solution was so dilute as to be beyond recognition. All unrecognized solutions were classified as water.

We chose for our tests the following typical substances. The strength of the initial solution of each is given below.

1. (Bitter) quinine, one part in 10,000 parts of water.
2. (Sweet) cane-sugar, one part in 10 parts of water.
3. (Acid) sulphuric acid, one part in 100 parts of water.
4. (Alkaline) sodium bicarbonate, one part in ten parts of water.
5. (Saline) sodium chloride, one part in 100 parts of water.

The attempt was made to include other substances, as aromatics, in the test; but it was soon found that the odor betrayed their presence without the aid of the sense of taste.

Other investigators have added astringents as a sixth class, but these substances are so often recognizable by odor, color, or some special taste not purely astringent, that it was thought best not to include them.

Tests by the method above described were made upon 128 persons; 82 being male, and 46 female observers.

The following table shows the amount of each substance which could be detected by the average observer:—

Substances.	Male Observers detected.	Female Observers detected.
Quinine . . . .	1 part in 302,000	1 part in 456,000
Sugar . . . .	" " 199	" " 204
Acid . . . .	" " 2,080	" " 3,280
Soda . . . .	" " 98	" " 126
Salt . . . .	" " 2,240	" " 1,930

From the above results the following conclusions may be drawn:—

1. The sense of taste is vastly more delicate for bitter substances than for any others. It is possible to detect quinine in a solution that is only  $\frac{1}{2000}$  the strength of a sugar solution, and we have previously shown (*loc. cit.*) that quinine is only  $\frac{1}{10}$  as bitter as strichnine.
2. The order of delicacy is, bitter, acid, salt, sugar, and alkali.
3. The sense of taste appears to be more delicate in women than in men. This is true in the case of all the substances excepting salt. As we had found a similar difference in favor of female observers in an earlier and independent set of experiments, which agreed in every essential particular with the results of the present test, we do not regard it as an accidental difference, or as likely to disappear in more extended investigations.

Marked differences in the delicacy of the sense of taste of different individuals were met with in the course of these experiments.

<sup>1</sup> Paper read at the New York meeting of the American Association for the Advancement of Science, August, 1887.

<sup>2</sup> See Relative Bitterness of Different Bitter Substances, by E. H. S. Bailey and E. C. Franklin, in *Proceedings of the Kansas Academy of Sciences*, 1885; Relative Sweetness of Sugars, by E. H. S. Bailey, in *Report of Kansas Board of Agriculture*, 1884; The Sense of Smell, by E. L. Nichols and E. H. S. Bailey, in *Nature*, xxxv. p. 74.